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Atmospheric
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Interactive comment on "Radar-radiometer retrievals of cloud number concentration and dispersion parameter in marine stratocumulus" *by* J. Rémillard et al.

Anonymous Referee #4

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General comments

The authors present a variation of the combined radar-microwave radiometer microphysical retrieval method of Frisch et al. 1995 (F95 hereafter). F95 retrieve parameters for cloud droplets as well as drizzle drops in the same column and describe their method as working in drizzling and non-drizzling conditions. The new method works only in non-drizzling conditions – which the authors are evidently aware is a rare condition for marine stratocumulus – and could be considered a step backward from F95. However, a step forward here is that instead of assuming the dispersion of the size

distributions, as done by F95, the new method retrieves dispersion, which removes a substantial source of uncertainty relative to the F95 retrievals, at the price of being applicable under very limited and possibly rare conditions.

The authors use the new method on two case studies and compare with another, independent remote-sensing technique, with seemingly favorable results, but the comparisons raised questions for me, which are included in my specific comments below.

Major comment

A fundamental assumption in the new method is that cloud droplet number concentrations vary with height and the dispersion – as measured here by log(sigma_g), the logarithm of the geometric mean radius of a log-normal distribution – is independent of height in the cloud. They do cite one reference (Miles et al. 2000) to justify this approach, but the assumptions are highly questionable. The authors only consider non-drizzling cases and stay away from cloud edges, so the only microphysical processes at play should be condensational growth and evaporation. But it is a textbook principle in cloud physics that as a distribution of cloud droplets grows through condensation, the distribution narrows, and conversely broadens during evaporation. At best – in terms of the assumptions here – the standard deviation of the size distribution is constant when surface tension is taken into account (Srivastava 1991), but even in that case the relative dispersion decreases with height in a cloud layer. (Relative dispersion is not identical to log(sigma_g), but the same sort of vertical trend holds.) It is not difficult to find in-situ studies that show relative dispersion and its variants increasing with height in stratocumulus clouds, for instance Wood 2000 and Lu et al. 2007, but there are many others. Wood 2000 also shows that the k parameter, which can easily be cast in terms of log(sigma_g), increases with droplet mean volume radius, which increases with height in a non-drizzling cloud. Similarly, Geoffroy et al. 2010 show that sigma_g increases with cloud water content, which also increases with height in stratocumulus clouds. It is easy to show that these two studies agree that log(sigma_g) should decrease by about 30-40% over the depth of a typical stratocumulus cloud. It is also widely found (perhaps starting with Nicholls and Leighton 1986) that droplet number concentrations are roughly constant with height in stratocumulus clouds, at least away from the edges, which is the only region analyzed here. It seems rather problematic that the new retrieval method makes assumptions that contradict first principles and many measurements that support them. Such a surprising assumption merits an extensive justification, in my view. Are the retrievals of droplet concentration and cloud optical thickness different if the more conventional assumptions of vertically uniform droplet concentration and dispersion increasing with height?

Minor comments

1. I would insert "non-drizzling" in the title before "marine" since it is such a fundamental limitation on this method.

2. As I understand it, the F95 method works on drizzling and non-drizzling clouds, so "drizzling and" should be inserted before "nondrizzling" [sic] in line 4 on p. 7508.

3. I would mention evaporation in downdrafts on line 15 of p. 7509.

4. The sentence starting on line 21 of p. 7509 should be removed, since drizzle occurrence does not limit the applicability of the F95 technique, which retrieves parameters for cloud droplets and drizzle drops in the same column.

5. In section 2 there should be references provided for the instruments used.

6. The Wood and Hartmann 2006 expression for effective radius is not the column average, as described here. Instead it is the cloud-top value, which Wood and Hartmann needed because they were using remote-sensing measurements from above. This misinterpretation likely has a bearing on the retrieval comparisons that appear later in the manuscript.

7. Confusing that on p. 7511 that "the liquid is distributed in the cloud layer using the Frisch et al. (1998) method" when so much in this new method seems to involve the profile of cloud water. Why is the new method not used to distribute cloud water

vertically?

8. The last sentence of section 2, regarding two-way attenuation, needs some further explanation or at least should cite a reference.

9. Definite integrals require upper and lower limits, throughout.

10. Unclear what is meant by "vertical resolution of in situ measurements is usually coarse". I can only guess that statement applies to coarsely averaged measurements. There is nothing intrinsic to in-situ measurements that makes them vertically coarse.

11. I can't make sense out of the sentence immediately following equation 13. Perhaps "appears" should be "disappears"?

12. It is stated the the profile of N_cld must remain close to its column-averaged value. It should be stated how close – one percent, ten percent, what?

13. It should be stated how rogue radar volumes are defined and how close to the edge is too close, and how cloud edges are defined.

14. The description of the treatment of mixing above the reflectivity profile on p. 7516 is too brief to be understandable. Further description and justification are needed.

15. The wavelength of the radar should be provided as should the vertical and temporal resolution of the retrievals.

16. It seems odd to refer to LWP that varies by a factor of two over a couple hours as being stable. I would describe that instead as varying substantially.

17. In the text or caption of fig. 1 there should be an explanation of why the radiometer retrievals twice disappear for a few minutes.

18. Given that the two optical thickness retrievals are independent, I don't understand this statement: "since the LWP variability drives to a large extent the [cloud optical thickness] variability, it is not surprising that the radar-radiometer derived and the shortwave

derived optical depths agree in the observed scales of variability". If the idea is that it is not surprising that both retrievals appear to agree so well, why is that not surprising? Because it was obvious from the start that both retrievals would be correct and thus agree? To many readers this may be quite surprising.

19. I would insert "and dispersion" after "radius" on line 25 of p. 7518.

20. Why are the effective radius retrievals so different between the NFOV and the new method, particularly when the optical thickness retrievals agree?

21. How is effective radius computed for the NFOV retrievals, given that the paper describing the NFOV retrievals does not mention effective radius retrievals?

22. Some explanation or justification or both are needed for this statement: "the statistical LWP retrieval was used instead of the physical one". Is this only for the 29 June case or for both cases? Why not use just one LWP retrieval method throughout?

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 7507, 2012.